AN ANALYSIS OF MEANING-VARIANCE IN POST-KUHNIAN PHILOSOPHY OF SCIENCE

ABSTRACT

Ph. D. THESIS

IN

PHILOSOPHY

BY

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Abstract

Realizing the ultimate inadequacy or irrelevance of logical positivism in understanding the growth of science, Kuhn sets about the task of giving a genuine explanation in which theory change comes about. One of the chief criticisms that Kuhn and others have raised against logical positivism is that the empiricist picture of the structure of scientific theories is unrealistic, it does not reflect the way in which science is actually done. While in the work of Kuhn much historical evidences lie in the background to articulate his account. The model of science, which Kuhn sees, has been explicated in terms of his notions of Paradigm. Kuhn characterizes a period of time during which a particular scientific community shares a paradigm as a period of normal science. During such a period, the energies of the members of the community are given over in solving puzzles defined by the paradigm which is itself based on some significant scientific

achievements. Further Kuhn argues that a transition from one paradigm to another involves a change in meaning, which establishes the endeavor that the scientist with a new paradigm sees differently from the way he has seen before.

Logical positivists hold that meaning of a scientific term remains invariant across a theory. On the contrary Kuhn was of the view that meaning of a scientific term varies through paradigm shift. Thus he establishes the positive and salutary virtue of his notion of paradigm shift. The meaning variance thesis has been a reaction to logical positivist's tradition, which holds that change, and comparison of meaning of scientific terms presented no problem.

The author in this thesis has made the modest attempt to analyse the problem of meaning variance or conceptual change, which has been dominated so much in post positivistic philosophy of science. Because the creation of concept through which to comprehend and communicate about physical phenomena constitutes much of the scientific enterprise. Thus it has been explored in Chapter-II that meaning variance according to Kuhn occurs in the form of paradigm shift.

It is desirable according to the topic to analyse the views of the few eminent post-Kuhnian thinkers such as Imre Lakatos, Larry Laudan, C R Kordig, Mark A Stone and John Watkin to see their approaches and findings. The chapter-III entails the respective position in the light of Kuhnian notion.

Lakatos begins his reworking on Kuhnian paradigm into research program and argues that the rationality of science lies in scientific research program and maintains that the relative merits of research programs can be compared and assessed. His research program methodology is concerned with how a mature science develops a particular theoretical prospective on the world. He defines the research program in terms of problems shift, which is said to be progressive if it is both theoretically and empirically progressive, otherwise it is degenerative. Thus Lakatos established that a mature and rational way of doing science is to undertake a research program, developing a theoretically progressive problem shift by trying to modify theories by the addition of new hypothesis.

In response to Kuhn's assault on the traditional philosophy of science and Lakatos' alternative view about the progress in science through research program, Laudan developed his model in the form of research tradition, which provides a guideline for the development of specific theory. The primary function of a research tradition is to establish a general ontology and methodology so that the rational persuitability of it may be determined by the rate of progress it has exhibited. Regarding meaning change as advocated by Kuhn, Laudan says that Kuhn's view leads to argue that history of science is nothing but a succession of a different world view and the rational change can never be made between such divergent schemes of the universe because each has its own rationale and integrity. Keeping the view of both positivists and post positivists regarding comparison of theories, Laudan holds another view and says that neither correspondence rule nor a theory-true observation language is necessary and established that we can compare theories with respect to internal consistency or coherence.

Now comes the brief view of the eminent post-Kuhnian thinker taken in this work is C R Kordig who maintained his stand against the meaning variance and incommensurable position of Kuhn by sketching six methodologically undesirable consequences. Finally Kordig established an alternative account and suggested that comparison of different theories are possible which are made through appeal to shared principles and meaning at first and second level.

Mark A Stone has also been discussed as a post Kuhnian and he objected to Kuhn's notion of paradigm shift (criteria for meaning change). As stated by Kuhn, the possession of paradigm as well as the decision to reject one paradigm and to accept another is a necessary condition for practicing science. Further the scientist never abandons one theory until a successor is at hand. Mark A Stone established a sharp opinion and exhamplifies several cases where scientist can and must reject one paradigm without ready successor.

The last eminent post Kuhnian thinker who has been endorsed in the present work is John Watkin who confronted with Kuhn's account and raised his objection about the possibility of the emergence of a new paradigm; (criteria for meaning variance) and suggested that paradigm monopoly, clash between old and new paradigm must go.

Thus there is no lack of suggestions for the criteria used by scientists in making their choice. At last, in my analysis, I find Kuhn's approach of paradigm shift; a dependent criteria for meaning variance is more successful, more revolutionary and may provide enough avenues and insights to the coming philosophers of science in their major concern of investigations.

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The present thesis entitled; **"An Analysis of Meaning-Variance in Post-Kuhnian Philosophy of science"** is a research work carried by Mr. Quaisar Shakeel under my supervision.

The topic has been assigned to Mr. Shakeel by the committee of Advanced studies on Research, Faculty of Arts, in its meeting held in the month of Dec'97.

I recommend that he may be allowed to Supplicate for the award of Ph.D.

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CONTENTS

Chapter-1

Chapter 1

INTRODUCTION

The Philosophy of science is an emerging area and can be traced back to Plato[1] and Aristotle[2]. With the rise of science in the sixteenth and seventeenth century, scientific thinking expanded on the question of how experiment and hypothesis could lead to knowledge. The most influential contribution of the new science was Francis Bacon's[3] "Novum Organum" first published in 1620, which formulated rules for discovering causal laws from experimental observation. Through the seventeenth and eighteenth centuries, controversies about scientific views were intermixed with controversies about methodology, for example debate between Cartesian and Newtonian about celestial mechanics and the appropriate role of hypothesis.

In the first half of the nineteenth century, three major works defined the philosophy of science. In 1830, John Hershel published his preliminary discourse on natural philosophy which accommodated both Baconian discovery of causal laws from observations and the use of hypothesis that go beyond what is observed. William whewell's philosophy of the inductive science published in 1840 contained insightful discussions of the importance of scientific concepts and explanatory theories. But in 1843 J S Mill published first of many editions of his system of logic which proved to be more in keeping with the empiricist temper of the time. Then in 1920 and 1930 there emerged a school in Europe consisting of very talented philosophers of science known as "Vienna Circle" advocating a doctrine that come to be known as "Logical Positivism". It is often said that Positivism emerged as a response to the metaphysical excesses of Hegel and his neo Hegelian successor who sought to explain reality in terms of metaphysical entities, which did not admit of empirical specifications.

Most of the logical positivists hold that scientific theories are to be understood as set of axioms in formal deductive systems. Theories are confirmed by deciding their consequences from the axioms and checking to see whether the predictions hold. This methodology is called Hypothetic Deductive because it emphasizes the use of hypothesis to make predictions.

Popper[4] (1959) was agreed with logical positivists on the issue of hypothetical deductive reasoning, however he differed on the role of prediction to be used to falsify theories, not to confirm them. The schema may be expressed in the following way.

Start with hypothesis "H".

Use logic to deduce predicted observation "O".

If "O" is observed then "H" is confirmed (Hempel).

If "O"is not observed "H" is falsified (Popper).

The logical positivist movement had great influence in philosophy and science but in the late 1950s it came under the severe attack to some of its central tenets.

Toulmin[5] (1953) & Hanson[6] (1958) criticized the hypothetical deductive account of theories and argued that theory and observations were much more intertwined then empiricist allowed.

In 1962, the first edition of <u>Thomas Kuhn's structure of scientific revolution</u> appeared and became the most influential work in the philosophy of science of the succeeding decades. Kuhn talked of paradigm, a conceptual scheme that governs not only how we see the world but even in some of his pronouncement how the world is. Kuhn, Feyrabend and others used historical analysis to show that elegant analysis of scientific theories that logical positivists offered bear little relation to scientific practice.

Today's philosophy of science is characterized by variety of approaches. In methodology, some philosophers look more to history, others to logical analysis.

I have not taken anything new but a general survey of logical positivism that how it emerged and its consequence upon the philosophy of science thereof.

Revolutionary new views concerning science have been advanced by Feyrabend, Hanson, T.S. Kuhn, Toulmin and others. Each holds that transition from one scientific tradition to another force radical change in

(a) what is observed.

(b) In the meaning of the terms employed.

There is a much similarity in the views of these thinkers. Each adopts the meaning variance position with regards to most scientific transitions. Their interpretations of science would seem to eventuate in relativism. It would become impossible as a consequence of their views to compare any two different scientific theories. Scientific transition would become complete and incommensurable replacement. So the new philosophy of science and their approaches brought out such features of science, which clearly conflict with traditional forms of logical empiricism. The logical empiricist tradition has tended to view the history of science as virtually irrelevant to the philosophy of science. It tended to look on the history of science as a chronological record of the slow removal of the obstacles to scientific progress.

Further logical empiricists resorted in particular to the presumably neutral and meaning invariant observational language employed by different theories. On the other hand radical meaning variance theorists have rejected this notion.

In short we can say that the logical empiricist traditions have over emphasized the invariance of meaning in scientific change. Actual science however does not proceed in this way. After scientific revolutions, scientists do use scientific terms in some new ways. One can say that the terms employed by successive theories have changed meaning to some degree.

In accordance with the topic of my thesis, I would like to choose first to put the Kuhn's account of the dynamic of scientific growth followed by the problems of meaning variance and incommensurability.

Kuhn describes scientific discovery as a sequence of normal science, crisis, revolution and new normal science. We can sketch the dynamics of scientific growth in the following way

Scientific growth->paradigm->Normal science->Puzzle solving->Anamoly-> Crisis->Extraordinary science-> Re-volution->Normal science.

In normal science, scientist revises theories for resolving anomalies using fixed paradigm and a fixed language. But from time to time, however, crisis occurs when scientists fail to cope with the anomalies observed. When crisis occurs, a revolution is needed in scientific investigations followed by a paradigm shift in which new language is adopted after which a new normal science starts. So we can say that Kuhnian notion of scientific growth is the development of new paradigm and its competition with the older one constitutes a scientific revolution. Kuhn further went to say that the rival paradigms are incommensurable;[7] means scientists in each paradigm would not be able to engage in rational dialogue across the boundary. Scientists in different paradigm according to Kuhn live and work in different worlds. So the introduction of the new paradigm paves the way for the creation of concepts through which to comprehend, to communicate about physical phenomena, constitutes much of the scientific enterprise. Concept plays a central role in the construction and testing of the laws and the principles of a theory. The introduction of the new concepts and the alterations of the existing ones are a crucial state in most changes in theories.

Thus our understanding of science is seriously deficient if we fail to examine the question of how scientific concepts emerged and are subsequently altered. This is especially notable in view of the fact that problems of conceptual change in science in the form of the "Meaning Variance" have dominated so much in post positivistic philosophy of science. So my endeavor in the present work would be to examine in particular the process of meaning change. If one throws an eye in the post positivistic philosophy of science, one can find that change of meaning is the result of scientific revolution i.e. it takes place in such a way that the concepts of the new theory completely replaces those of the previous. But in the realm of the positivist's framework, account of meaning and meaning change in science erected no problem. Now I have to concentrate on my problems related to the topic to reach up to the solution of the problem, I [illegible] first take up the Kuhnian notion of meaning change.

The second chapter will explore how Kuhn differs from Logical Positivist's account of meaning Invariance and in what respect he propounded his theory of meaning variance in the form of paradigm shift.

In the third chapter, the author wishes to analyze the notion of few eminent post Kuhnian thinkers such as Imre Lakatos, Larry Laudan, C R

Kordig, Mark A Stone, John Watkin to see how they established their position in comparison to Kuhnian Notion.

At the stage of concluding my work, I argue that the Kuhn's model of Meaning Variance in the form of paradigm shift is highly appealing and it may provide a major breakthrough in scientific progress.

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Chapter-2

CHAPTER-2

Thomas Kuhn's Notion of Meaning Variance

Logical empiricists have treated scientific theories as semantically interpreted formal system with precise rule of interpretation. They seldom analyze or examine the historic developments of actual science. This tradition has tendered to view the history of science virtually irrelevant to the philosophy of science.

As Shapere stresses, the positivistic tradition within twentieth century philosophy of science has not only utilized the notion of a meaning invariant observational language in order to analyze the meaning and acceptability of Single theories. It also uses this device by which different theories can be said to be "in competition" and as a basis by which one theory can be chosen as better than its competitors.[1]

This was the alleged source of reaction to the Meaning Variance theorists particularly Kuhn. He has emphasized the variance of meaning in scientific change and claimed that different theories are incommensurable in meaning.

Kuhn views science as working from within a perspective or Weltanschauung which shapes the interest of the science, how phenomena are viewed, the demand it makes on theories, and the criteria of acceptability it insists on for theories. He argues the evolution of scientific Weltanschauungen as fundamentally discontinuous, which amounts to the rejection of one Weltanschauungen in favour of another. With this change some of the old theories, laws, the results are rejected and those, which are not rejected, are reinterpreted or modified when incorporated into the new Weltanschauungen. So Kuhn views major scientific advances as being revolutionary in nature.

In his most influencing work, "The structure of Scientific Revolution", Kuhn has as his basic problem; the nature of scientific change and he summarizes that "scientific revolutions are those non-cumulative developmental episodes in which an older paradigm is replaced in whole by an incompatible new one,[2] where paradigms are defined to be "accepted examples of actual scientific practice which include law, theory, application and instrumentation together to provide models from which spring particular coherent traditions of scientific research. [3]

Kuhn admits that his use of paradigm identifies two quiet distinct notions; exemplars which are concrete problem solutions accepted by the scientific community and disciplinary matrixes which are the shared elements and have as components, symbolic generalization, shared commitments to beliefs in particular model, shared value and shared exemplars. If scientific changes are fundamentally revolutionary, there must be non-revolutionary periods as well,[4] and Kuhn's starting point is to characterize the nature of non-revolutionary science or he calls it Normal science.

The characteristics of normal science is that it is carried out by a scientific community which shares firm answers to questions like the following: what are the fundamental entities of which the universe is composed? How do these interact with each other and with what sense? What questions may be legitimately asked about such entities and what techniques employed in seeking solutions? [5] The additional characteristics of normal science is that members of scientific community share a common disciplinary matrix. Normal science is viewed as the routine verification of the dominant theory in any historical period. Verification and testing become part of a puzzle solving activity. In Kuhn words:

> "Normal Science" means research firmly based upon one or more past scientific achievements, achievements that some particular community acknowledges for a time as supplying the foundation of its practice. Today such achievements are recounted, though seldom in their original form, by science textbooks, elementary and advanced. These textbooks expound the body of accepted theory, illustrate many or all of its successful applications and compare those applications with exemplary observations and experiments. [6]

Kuhn thus keeps his position by saying that normal science is carried out by scientific communities bound together by a common disciplinary matrix, which is acquired through an apprenticeship characterized by the exemplars shared by the scientific as being archetypal of good science.

According to the logical empiricist's tradition, the empirical or observational content of the symbolic generalization in a theory is fully or partially specified by correspondence rule which explicitly states the allowed methods for attaching the generalization to phenomena and also supply the various theoretical terms in the generalization with their empirical interpretation or meaning. Kuhn rejects this account, arguing instead that a new theory is always announced together with exemplary applications to some concrete range of natural phenomena. [7] That is, a theory is always, advanced in conjunction with various exemplars which are presented as an archetypal application of the theory to phenomena. Furthermore, since exemplars indicate the sorts of questions to be asked and the kinds of answer to be given, different communities with different stocks of shared exemplars will disagree on what questions ought to be asked and what count as solutions to these questions. So in short, they disagree on what constitutes good science even if they are concerned with the same phenomena. That is depending on one's exemplars one has different scientific value. What are characteristics of a scientific community, then, is a commonly held disciplinary matrix, which is acquired by the mastery of the scientific community's shared stock of exemplars. Normal science then is the science practiced by a scientific community whose common possession is a disciplinary matrix based on a shared stock of exemplars. Normal science is occupied with solving the open-ended problems or puzzles posed by the exemplars and disciplinary matrix based upon them.[8]

As such it is highly cumulative enterprise devoted to augmenting the critical success of the exemplars in dealing with the problem area or class of phenomena. It's task being the extension and further articulation of the disciplinary matrix through the production of additional exemplars and the refinement of existing ones. It does not aim at the production of novelties of fact or theory rather its aim is to show that nothing is novel, that everything is in accordance with its generalization as interpreted by the disciplinary matrix.[9] It is the attempt to subsume an increasingly larger class of phenomena under the basic world view supplied by the evolving disciplinary matrix.

But a time comes when normal science fails to accord with its expectation and anomalies are discovered which results a scientific crisis. Such crisis sets a stage for scientific revolution. So the scientific revolution is a stage when the worldview supplied by normal science and its shared disciplinary matrix no longer seems adequate to cope with all the phenomena. Thus the scientific community begins to loose faith though they do not renounce the theory that led them into crisis. So the decision to reject one theory for another is always simultaneously the decision to accept another. Thus, before a disciplinary matrix is rejected, a replacement must emerge and the scientific revolution consists in the switch of allegiance from the old to the replaced one. Kuhn calls this replacement as paradigm shift.[10] This replacement will be the product of extraordinary research. Different scientists work from within different ones of the proliferating disciplinary matrices, each using different rule for research which means that different scientists look at the field in different ways. The proliferation of competing articulation, the willingness to try any thing, the expression of explicit discontent, all these are symptoms of a transition from normal to extraordinary research.[11]

Kuhn says that the old theory and the replaced one must be logically in compatible. To accept the new theory is to accept the new symbolic generalization and certain applications of these generalizations as archetypal exemplars. Since the exemplars implicitly interpret the symbolic generalization and determine the meaning of the theoretical terms occupying in them, accepting the new theory requires accepting a new or altered vocabulary for viewing the world. So this change constitutes accepting or acquiring a new disciplinary matrix. So acceptance of new theory requires rejecting the old disciplinary matrix in favor of another. If the comparison does show the new theory better able to accommodate the phenomena than the old, then switch of allegiance to it and its associated disciplinary matrix by an increasing proportion of scientific community may result.

Thus the scientific revolution is completed when most of the scientific community has switched allegiance to the new disciplinary matrix, at this point the scientists once again bound together by common disciplinary matrix and are engaged in doing normal science.[12]

How do the possessors of the new disciplinary matrix differ from the old one? The new matrix may possess some of the old symbolic generalization, but will do so with changed meanings attached to the theoretical terms. For example, a relativity theory still employs classical equation of motion, but with different meaning, the equation being only approximations of limited scope to the generalization of relativity theory, but they are not the old classical laws since the key terms, "mass" and "force" now have the new meaning of relativity theory. Hence, when revolution occurs the scientific advancement which results is not cumulative, rather it is a fundamentally reorientation of the science which requires rejecting the old science for new.

The conceptual changes, which come from accepting a new disciplinary matrix, are a gestalt switch; two observers looking at the same things from within different disciplinary matrixes see different things. Though the world does not change with a change of disciplinary matrix, the scientists afterwards work in a different world.[13]

So we see that Kuhn's account of science fundamentally depends on the distinction between normal science and revolutionary science and his account of revolution as involving conflict between incommensurable disciplinary matrixes.

Kuhn says; " The normal scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.[14]

There are number of sources for the doctrine of incommensurability. In order to bring them into focus I will consider briefly the chief sources.

The first source of incommensurability will be referred to as incommensurability due to value variance. Kuhn takes it that in some cases the disagreement between scientists as to which of a pair of theories to prefer arises from disagreement about values. In his postscript, he writes:

> What it should suggest, however, is that such reasons [accuracy, simplicity, fruitfulness and the like] function as values and men who concur in honoring them can thus differently apply them, individually and collectively. If two men disagree, for example about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of

fruitfulness and, say, scope in reaching a choice, neither can be convicted for a mistake. Nor is either being unscientific. [15]

Kuhn is advancing two claims, which if accepted would generate some incommensurability. Where incommensurability is understood as indicating a limitation on the possibility of making rationally justifiable choices between theories. The first claim is that in justifying my preference for one theory over another, I shall have to appeal to value judgement. The second is that value judgements are autonomous in the sense that no rational considerations can be adduced for favoring one value judgement over another.

A second related source of incommensurability would arise if in some cases of scientific conflict, the rival scientists disagreed as to the principles of comparison and there was no possibility of rationally justifying one of these sets of principles over the other even though the difference did not arise from a disagreement over values. In this event there would be cases of incommensurability in the sense that there would be no possibility of adducing rational consideration favoring one theory over another. This source of incommensurability is called as the incommensurability due to radical standard variance.

The most extreme and most interesting source of incommensurability is called as radical meaning variance. It would be fruitful to start with a relatively modest version of the thesis that will be called the thesis of radical meaning variance of theoretical term. For the sake of the argument let us suppose that there is an observation- theory dichotomy and consider the radical meaning variance theory to be the thesis that the meaning of a theoretical term with in a theory may change if certain alterations are made in that theory. Suppose, for instance, that the change from Newtonian mechanics to relativistic mechanics was of such a character that the meaning of "mass" changes radically. In this case there is only the appearance of logical incompatibility between the Newtonian assertions that, say mass is invariant and the relativistic assertion that, mass is not invariant. Before developing further the consequences of admitting this meaning variance, it will be instructive to consider the chain of reasoning that gave rise to the thesis of the radical meaning variance theory. To see this we need to begin with some reflections on the problems of the meaning of theoretical terms. In the positivistic tradition, theoretical terms were taken to be particularly problematic from the semantic point of view. Theoretical terms were problematic for, since they were not applied to items directly given in experience as in the case of observational terms, which could be specified directly through their connection with experience. To understand the meaning of an observational predicate was to grasp the kinds of experience that constituted evidence for the application of a predicate and to grasp the kind of experience that constituted evidence against the application of the predicate, whereas in the case of theoretical terms their meaning could be specified in terms of antecedently understood observational terms. For, theoretical terms were to play a role in the explanation of observations and it was required to play a role in a theory. The response of this situation was to suppose that the meaning of the theoretical terms (T-terms) was implicitly defined through some of the postulates that are meaning postulates of the theory. Some postulates in this set would connect a theoretical term with other theoretical terms within the theory. Other postulates would connect the theoretical term with observational term. On the meaning postulate approach it specifies concerning the meaning of theoretical terms. Consequently not all changes in theory involve changes in the meaning of theoretical term across the theories. For, on this account we might have two incompatible theories having the same meaning postulates set for theoretical terms. Thus the meaning postulate approach provides us with a framework within which it makes sense to ask if a given theoretical terms means the same within theory T-1 as it does within theory T-2.

Thus the meaning postulate approach applied to theoretical terms but not to observational terms provides us what we call the first degree of meaning variance. In these change theory changes, such as shift from Newtonian to Einstenian theories of mechanics does not necessarily produce changes in the meaning of theoretical terms. Whether there is change in meaning depends on the particular changes made in the theory. In the philosophy of Carnap one can find the approach about the meaning variance of theoretical terms as the quotation below reveals:

> Perhaps the objection might be raised that, if significance is dependent upon theory (T) then any observation of new fact may compel us to take as non-significant, a term so far regarded as significant or vice versa. This class will generally be changed only when especially the introduction of a new theoretical and the addition of postulates make a radical revolution in the system of science, for that term.[16]

Thus Carnap acknowledged the first degree of meaning variance and in doing so came to hold a view not dissimilar to Kuhn. For Kuhn maintained that meanings vary only across dramatic theory changes. If one rejected the assumption that there is a distinction in kind between meaning postulate and non-meaning postulate while retaining the general idea of the meaning postulate approach, that meaning of theoretical terms is to be specified through a specification of the role of the term in the theory, one would lead to embrace the second degree of meaning variance. The thesis of the second degree of meaning variance is that the meaning of all the theoretical terms changes under theory change while meaning of observational terms remains constant.

In discussion of incommensurability there has been a tendency to focus on the question of meaning. Having reached the second degree of meaning variance we can articulate what the problem of incommensurability has been supposed to do. Taking Einstenian and Newtonian theories as our examples, the problem can be posed as follows. Some of the theoretical assertions of Newton seem incompatible with some of the theoretical assertions of Einstein. For example Newton says mass is invariant and Einstein says mass is not invariant. These assertions represent mere equivocation. In fact the situation would be more perspicuously represented as follows:

Mass-N (Newtonian mass) is invariant, mass-E (Einstenian mass) is not invariant. Since both the mass here differ in meaning, these later assertions are not logically incompatible. If this applies to all theoretical terms Einstein and Newton are not contradicting. At the second degree of meaning variance it is taken that observational term has invariant meaning across theory change. This would give a way, which theories could be, contradictories of one other notwithstanding the variation in the meaning of the theoretical terms. The assumptions that there is a difference in kind of either an epistemological character or a semantical character between so called observational statement and so called theoretical statement is untenable.

Given the second degree of meaning variance, rejecting the observational- theoretical distinction leads to third degree of meaning variance. In this third degree it is taken that the meaning of all terms is determined through their role in a theory with the consequence that any change in the theory brings a change in the meaning of all terms. But for Kuhn, we have the third degree of meaning variance only in the case of **paradigm shift.** So the model of science which Kuhn sees has been explicated in terms of his notion of a paradigm.

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Chapter-3

Chapter 3

Post kuhnian philosophers of science

In this chapter, the author is going to discuss some post Kuhnian thinkers such as Imre Lakatos, Larry Lauden, C. R. Kordig, Mark. A. Stone, John watkin.

Imre Lakatos:

In the previous chapter, it has been explored that "meaning variance" occurs in the form of "paradigm shift". Lakatos was not interested in question of meaning as such and for this reason he did not take up the challenges by the arguments for incommensurability. Rather he was particularly concerned with the question of how he could vindicate his principles of comparison (his methodology).

According to his methodology the great scientific achievements are research programs. The methodology of scientific research programs is to be used in making action guiding decision with regard to theory choice in contemporary science.[1] Lakatos says that research program can be evaluated in terms of progressive and degenerating problem shift and scientific revolution consists of one research program superseding another. But a research program is said to be progressive as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success. In other words, it can be said that within a research program a theory can only be eliminated by a better theory, by one which has excess empirical content over its predecessors. Though his methodology of research program was criticized by both Feyerabend and Kuhn. According to Kuhn, Lakatos must specify criteria which can be used at the time to distinguish a degenerative from a progressive research program and so on[2] otherwise he (Lakatos) has told us nothing at all. Lakatos otherwise says that he begins his reworking of Kuhnian paradigm into research program and sees the rationality of science in his scientific research programs. The relative merits of which can be compared and assessed. He defined research programs in terms of problem shift. Let T_1, T_2, T_3, --------------- be a series of theories where each subsequent theory results from the semantically reinterpretation of the previous theory in order to accommodate some anomaly, where each theory in the series has as much empirical content as the unrefuted content of its predecessor. The problem shift is said to be progressive if it is both theoretically and empirically progressive, otherwise it is degenerating.[3]

In mature science he says, the series of theories are generated in accordance with research program having "heuristic power".[4] Such research programs consists of methodological rule for the development of problem shift; these rules comprise a negative heuristic that tells us what path of research to avoid and positive heuristic tells us what path to persue.[5] We can say in other words that criteria of positive heuristic power strongly depends upon how it construct factual novelty. Further Lakatos says that his research program may be characterized by their hard core. The negative heuristic specifies the hard core of the program which is irrefutable by the methodological decision of its proponents while the positive heuristic consists of a articulated set of suggestion or hint regarding the development of research program.[6]

According to Lakatos we are to compare theories by examining the track record of the scientific research program within which the theories are embedded in the hope that the past record is indicative of the future success rate. To do this we attempt to discover how successful the rival program has been in generating true novel predictions. A preliminary problem is that the explanation of a known fact can be as important in providing evidence for a theory as the generation of true novel predictions. Lakatos further does argue that his account of science and his methodology of research program is superior to other because the rationale involves is effective to solve the anomalies. The famous dictum is that "philosophy of science without history of science is empty; history of science without philosophy of science is blind."[7] For Lakatos, the history of science gives philosophy of science its content through providing the test between the rival methodologies.

> All methodologies function as historiographical or metahistorical theories or research program and can be criticized by criticizing the rational historical reconstruction to which they lead.[8]

That is we compare rival methodologies by comparing the different historical accounts (rational reconstruction) to which their use gives rise.

> Progress in theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstructing of a growing bulk of value impregnated history as rational. [9]

Lakatos argues that his methodology is superior and appealing to other methodologies because it is supposed to be used by the scientist and it must bear relation to what they in fact regard as the good making feature of theories. He further says that it would be wrong to assume that one must stay with a research program until it has exhausted all its heuristic power, that one must not introduce a rival program before everybody agrees that the point of degeneration has probably been reached. Although one can understand the irritation of a physicist when, in the middle of the progressive phase of a research program, he is confronted by a proliferation of vague metaphysical theories stimulating no empirical progress.[10] One must never allow a research program to become a Weltanschauung or a sort of scientific rigor setting up itself as an arbiter between explanation and non explanation, as mathematical rigor sets itself up as an arbiter between proof and non proof. Lakatos reacts here and says that unfortunately this is the position, which Kuhn tends to advocate. Indeed what he calls normal science is nothing but a research program that has achieved monopoly. Thus Lakatos establishes that the history of science have been and should be a history of competing research program. He further makes question that, can there be any objective reason to reject a program, that is to eliminate its hard core for constructing protective belts?

The answer lies is that such a objective reason is provided by a rival research program which explains the previous success of its rival and supersedes it by a further display of heuristic power. However the criterion of heuristic power strongly depends on how we construe factual novelty. But the novelty of a factual proposition can frequently be seen only after a long period has elapsed.

All the Lakatos' position suggests that we must not discard a budding research program simply because it has so far failed to over take a powerful rival. We should not abandon it, if supposing its rival was not there, it would constitute a progressive problem shift. And we should regard a newly interpreted fact as a new fact.

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Larry Laudan

So far we have seen the view of Lakatos related to the to the problem of meaning and how he out lined his research program in comparison to kuhn's paradigm shift, here are the findings of Laudan that how he has emerged with he has new account which was termed as Research tradition, a primary tool for understanding and appraising scientific progress.

Before I go into the details of Laudan's programs, I will see first how he looks the development made by both kuhn and Lakatos .

As we have seen earlier, kuhn offers a model of scientific progress whose primary element is the "paradigm"[1]. Once a paradigm is accepted by scientists, they can proceed with the process of "Paradigm articulation", known as normal science. In periods of normal science, the dominant paradigm will itself be regarded as unalterable and immune from criticism .It remains so until enough anomalies accumulate, then scientists begin to ask whether the dominant paradigm is really appropriate. Kuhn calls this time a period of crisis. During a crisis, scientists begin to consider seriously alternative paradigms. If one of those prove to be more empirically successful than the former paradigm, a scientific revolution occurs, a new paradigm is enthroned, and another period of normal science ensues. Here Laudan says that despite of all its strengths, Kuhn's model of scientific revolution suffers from acute conceptual and empirical difficulties. He then outlined the serious flaws in Kuhnian program, the most significant are below.[2]

1. Kuhn never really resolves the crucial question of the relationship between a paradigm and its constituent's theories. Do theories, once developed, justify the paradigm, or does the paradigm justify them? Laudan says, it is not clear in Kuhn's case.

2. Kuhn's paradigms have a rigidity of structure, which precludes them from evolving through the course of time in response to the weakness, and anomalies, which they generate. Moreover, he makes the core assumption of the paradigm; there can be no corrective relationship between the paradigm and the data. Accordingly, it is very difficult to square the inflexibility of Kuhnian paradigms with the historical fact.

3. Kuhn's paradigms are always implicit, never fully articulated. As a result, it is difficult to understand how he can account for many theoretical controversies, which have occurred in the development of science.

4. Because paradigms are so implicit and can only be identified by pointing to their exemplars (basically an archetypal

application of a mathematical formulation to an experimental problem), it follows that whenever two scientists utilize the same example, they are, for Kuhn committed to the same paradigm. Such an approach ignores the fact that different scientists often utilize the same laws or exemplars. To this extent, analyzing, science in terms of paradigms is unlikely to reveal that it is a strong network of commitments - conceptual, theoretical, instrumental which Kuhn hoped.

In response to Kuhn's assault on the traditional philosophy of science, Imre Lakatos has developed an alternative view about the progress in science and that is through research program. Laudan says that Lakatos' model is in much respect an improvement on Kuhn. Lakatos discusses the historical importance of the coexistence of several alternative research programs at the same time with in the same domain. Kuhn often takes the view that paradigms are incommensurable and thus not open to rational comparison. However Lakatos insists that we can objectively compare the relative progress of competing research programs. Lakatos' research program, like Kuhn's paradigms are rigid in their hard core structure. Moreover Laudan says that he is indebted pioneering work of Kuhn and Lakatos but tries to develop the notion of research tradition as an alternative model[3]. According to him, a research tradition provides a set of guidelines for the development of specific theory. Laudan says that every research tradition will be associated with a series of specific theories, each of which is designed to particularize the ontology of the research tradition and to satisfy its methodology. The mechanistic research tradition in seventeenth century optics for example, includes several of optical theories of Hooke, Rohault, and Huygens. Similarly the phlogiston tradition in eighteenth century chemistry received more than a dozen specific theoretical formulation. So the whole function of a research tradition is to provide us with the crucial tool we need for solving problem both empirical and conceptual.

There are at least two specific modes by which theories and research traditions are related:[4] one is historical and other is conceptual. It is a matter of historical fact tat most of the major theories of science have emerged when the scientist who invented them was working with in one or another specific research tradition. For example Boyle's theory of gases developed with in the framework of the mechanical understanding of the world. Hertz's electrical theories were linked in important ways with the Maxwellian research tradition. Thus a specific theory, abstracted from its historical context may not give unambiguous clues as to the research tradition with which it is associated. It is in the sense, the connection between a theory and a research tradition is as real as any fact of the past. In order to see how important these connections are, we need to look at the ways in with theories and research tradition interacts.

The problem determining role of the research tradition: Even before specific theories are formulated with in a tradition, the research tradition will strongly influence the range. So the research traditions have a decisive influence on what can count as the range of possible conceptual problems which the theories in that tradition can generate.

Further Laudan says that the primary function of a research tradition is to establish a general ontology and methodology for tackling all the problems of a given domain or set of domains.[5]

The research tradition can play a vital heuristic role in the construction of specific scientific theories. Considcr the case of Benjamin Franklin and his efforts to articulate a theory of static electricity. Franklin was familiar with certain phenomena (particularly electrification by friction, electroscopes, and the leyden jar). Working within a research tradition which postulated the existence of electrical matter, Franklin needed a theory which could explain how friction electrifies bodies, how electrical bodies could attract and repel, how electricity could be stored in a condenser and why certain bodies were conductors others were insulators. In the early stages of the development of his theory, Franklin came to the view that the positive electrification consisted in the accumulation with in bodies of an excess amount of the electrical fluid, while negative electrification was caused by a deficiency of this fluid. These specific theoretical assumptions are linked together with the ontology of research tradition, an ontology which postulated that electricity was a form of matter and therefore conserved in the same way that ordinary matter was, it became natural to assume that electrical charge must be conserved. This important theoretical insight subsequently confirmed in Franklin experiment and emerged as an inevitable result of Franklin's thinking about the relation between his theory and its parent research tradition.

Laudan also quotes another example. When Sadi Carnot set out to develop a theory of steam engines, he sought to do so with in the research tradition of caloric doctrine of heat. With in this tradition, heat was conceived as a material, conserved substance capable of moving between the constituent parts of microscopic bodies. Carnot, familiar with the work that could be performed by such simple mechanical system as a water wheel, tried to conceive of heat flow on analogy with the fall of water, with the temperature gradient between input and output corresponding to the top and bottom heights of the water fall. It is in terms of this analogy that Carnot develops the proof of his theory. Thus it is clear here that, if Carnot had not conceived of heat as a conserved substance capable of flowing from one point to another without loss of its quality, he could not have enunciated his theory. But that way of conceiving heat was natural result of the research tradition with in which Carnot worked. In both the cases above, the research tradition functions heuristically to suggest an initial theory for some domain.[6] Research traditions, as we have seen are historical creatures. They are created and articulated with in a particular intellectual milieu. They aid in generation of specific theories. But Laudan says that there is another important way in which research traditions evolve.

These changes involve not the specific theories with in the research tradition but a change of some of its most basic core elements. Laudan discusses this type of transformation in some detail since there are philosophers who have denied that research traditions are capable of any significant internal modification. For instance, both Kuhn and Lakatos usually suggest that entity such as research traditions have rigid and unchanging set of doctrines, which identify and define them. Any change in those doctrines produces a different research tradition. Laudan argues that we must reject it for it can create confusion in our effort to get some understanding of the historical processes of science. Laudan says that if one looks at the great research tradition in the history of scientific thought for example Aristotelianism, Cartesianism, Newtonianism, one can see that there is scarcely any interesting set of doctrines which characterizes any one of these research tradition through out the whole of its history. Certain Aristotelian, at times, abandoned the Aristotelian doctrine that motion in a void is impossible. Certain Cartesians repudiated the Cartesian identification of matter and extension. Certain Newtonian abandoned the Newtonian demand that all matter has inertial mass. So the core assumptions of any given research tradition are continuously undergoing conceptual scrutiny. In such circumstances, it is common for partisans of a research tradition to explore what sort of changes can be made in the ontology of that research tradition to eliminate the anomalies and conceptual problems confronting its constituent theories.[7]

Oftenly scientists find that by introducing one or two modification in the core assumption of the research tradition they can both solve the outstanding anomalies and conceptual problems. For this research tradition must be carefully evaluated to come to its hope for achieving the goal (problem solving adequacy). Laudan gives two quite different contexts.[8] One is the context of acceptance and another is the context of pursuit. In the former case, scientists often choose to accept one among a group of competing theories and research tradition. There is a whole range of possible answers here. Inductivists will say, choose the theory with the highest degree of confirmation or choose the theory with the highest utility. Falsificationists say, choose the theory with the greatest degree of falsifiability. Others such as Kuhn would say no rational choice could be made. But Laudan replies that choose the theory or research tradition with the highest problem solving adequacy. Thus Laudan establishes that the rationale for accepting or rejecting any theory is fundamentally based on the idea of problem solving. If one research tradition has solved more important problems than its rivals have, then accepting that tradition is rational precisely to the degree we are aiming to progress.

This way of appraising research tradition according to Laudan has three distinct advantages.

(1) It is workable unlike both inductivist and falsificationist model.

(2) It simultaneously offers an account of rational acceptance.

(3) It comes closer to being widely applicable to the actual history of science.

The context of pursuit; So far as we have seen an adequate account of theory choice, but according to Laudan we are still very far from pursuing a full account of rational appraisal. The research for this is that there are many important situations where scientists evaluate competing theories by criteria, which have nothing to do with the acceptability of the theories, in question. The actual occurrence of such situations has often been observed. For example when we look to Copernicanism, the early stages of the mechanical philosophy, the atomic theory in the first half of 19^{th} century, the preliminary efforts at the quantum mechanical approach to molecular structure. We see the same pattern that scientists often begin to pursue and to explore a new research tradition long before its problem solving success, qualifies it to be accepted over its older rivals. Since the central aim of science is to provide the solutions of a maximum number of empirical problems and anomalies. This view entails that we should accept at any time the theories or research traditions, which have shown themselves to be the most successful problem solvers.

Suppose we have two competing research traditions RT and RT^1 and the momentary adequacy of RT is much higher than that of RT^1 but the rate of progress of RT^1 is greater than the related value of RT. So far as acceptance is concerned, RT is clearly the only acceptable one of the pair but on the other hand RT^1 has shown itself to be capable of generating new solutions to problems. This would be appropriate if RT^1 is a relatively new research tradition.

Thus Laudan establishes that it is always rational to prove any research tradition which has a higher rate of progress than its rivals. So we see that rational persuitability of a research tradition is determined by the rate of progress it has exhibited. But what should be the guiding principles for rationality? Laudan has provided the following criteria.[9]

1. In the case of Competing scientific research traditions, if one of those traditions is compatible with the most progressive worldview available, and the stir is not, then there are strong grounds for preferring the former.

2. If both (competing scientific research traditions) can be legitimated with reference to the same worldview, then the rational decision between them may be made on entirely scientific grounds.
3. If neither competing scientific research tradition is competable with a progressive world view, their proponents should either articulate a new progressive world view which does justify them or develop a new research tradition which can be made compatible with the most progressive extant world view.

So, instead of defining progress in terms of rationality, he defines rationality in terms of progress. Laudan's expression has two main themes - that any adequate model of science must recognize and be able to accommodate scientific change and that rationality and progress are linked with the problem solving effectiveness of theories.

By taking into account of scientific change generated by revolutionary camp, (such as Kuhn, Feyrabend, and other) Laudan argues that their views leads them to conclude that the history of science is nothing but a succession of different world views and that rational change can never be made between such divergent schemes of the universe, because each has its own internal rationale and integrity, no meaning can be attached to the suggestion that one scheme is more or less rational than another.

If there are no conceivable grounds for rational choice between competing research tradition then science will bear unaccountability. This means that tradition, which happens to attract the most influential adherents, will become influential. But before one accept this depressing conclusion that science proceeds in this way, it is worth examining with some core and the arguments.

The central argument runs like this: Scientific theories implicitly define the terms, which occur within them. Hence, if two theories are different then all the terms within them must have different meanings. Thus when an Einstenian physicist refers to the "mass" of a particle, he means something different from a Newtonian when the latter refers to the "mass" of a particle. As a result, scientists working in different research traditions cannot communicate with, and cannot understand the statements of their fellow scientists in other tradition. Given this general in comprehensive, science emerging as a new version which employs that theories cannot be compared and rationally evaluated because such comparisons require a common language.

Laudan holds this above argument to be faulty in several respects[10]. It bags a number of questions about synonymy and translation. But its central flaw, for one purpose, lies in its presumption that rational choice can be made between theories only if those theories can be translated into one another's language or into a third, "theory-neutral language".

As Kuhn puts the point, "The comparison of two successive theories demands a language into which at least the empirical consequences of both can be translated without loss or change.

Laudan, on the contrary maintained that even if we accept the view that all observations are theory laden to a degree that makes their contents inseparable from the theory that is used to express them, it is still possible to outline machinery for objective rational comparison between competing scientific theories and research traditions. Laudan mentioned two arguments for such a conclusion:[11]

1. The argument from problem solving: Logical positivists argued that competing theories could be evaluated by comparing their observational consequences.

They usually conceived correspondence rule for the process of translating the competing theories into some purely observational language because observational language was held to be frame of any speculative theoretical basis. It was thought to provide objective grounds for the empirical appraisal of vying theories. As doubts grew about the existence of correspondence rule and about the theory-true observational language, philosophers from revolutionary camp such as Kuhn, Feyrabend and others suggested that theories were incommensurable and not open to objective comparison.

But in comparison of the above two arguments (logical positivist and post positivists) Laudan holds another view and says that neither correspondence rules nor a theory-true observation language are necessary for comparing the empirical consequences of competing theories. He says that without correspondence rulers and without a purely observational language we can talk meaningfully about different theories being about the same problem, even when the specific characterization of that problem is dependent upon many theoretical assumptions. If a problem can be characterized only within the language and the framework of a theory, which purports to solve it, then clearly no competing theory could be said to solve the same problem. However, so long as the theoretical assumptions, necessary to characterize the problem are different from the theories, which attempt to solve it, then it is possible to show that the competing explanatory theories are addressing themselves to the same problem. Consider a very elementary example. Since antiquity, scientists have been concerned to explain why light is reflected off a mirror or other polished surface according to a regular pattern. Relating the incident to the reflected angle, the problem of reflection thus characterized, involves many quasi-theoretical assumptions, such as; light moves in a straight line, that certain obstacles can change the direction of a ray of light, that visible light does not continuously fill easy medium etc. Does the existence of these theoretical assumption entails that no two theories can be said to solve the problem of reflection? The answers is clearly, provided that theories which solve the problem are not inconsistent with those relatively low level theoretical assumptions required to state the problem. Laudan says that he does not mean to suggest that all the problems which a theory or research tradition attempts to solve can be characterized independently of the theory which solves them. The determination of the independence of any specific problem must depend upon the particularities of the case.

He further says that there are far more problems common to competing research traditions than there are problems unique to a single one. These shared problems provide a basis for a rational appraisal of the relative problem solving effectiveness of competing research traditions.

Laudan criticized Kuhn and said that he has been misled by his discovery that some empirical problems are not jointly shared between traditions or paradigms into believing that no problems are identical.

2. The arguments from progress: It was observed that rationality consisted in accepting those research traditions which had the highest problems solving effectiveness. Now, an approximate, determination of the effectiveness of a research tradition can be made within the research

tradition itself, without reference to any other research tradition.

Laudan simply asks whether a research tradition has solved the problems which it set for itself, he further asks whether in the process, it generated any empirical anomalies or conceptual problems. In this way, Laudan says we can come up with a characterization of the progressiveness (or regressiveness) of the research tradition.

He says that, if we did this for all the major research traditions in science, then we should be able to construct something like a progressive ranking of all the research traditions of a given time. It is thus possible at least in principle and perhaps eventually in practice to be able to compare the progressiveness of different research traditions. He says that, even if we could not in principle ever find a way of translating Newtonian Mechanics into relativistic mechanics; if we could never find a way of comparing the claim of twentieth century particle physics with nineteenth century atomism, then it would still be possible to make an assessment on rational grounds of the relative merits of these research traditions. Thus Laudan established that we can compare theories with respect to their internal consistency or coherence and possible incommensurability of theories and research traditions does not preclude the existence of comparative appraisal of their acceptability.

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C R KORDIG

Among the post Kuhnian philosophers, C. R. Kordig also enjoys an important position. Kordig does repudiate the Kuhnian position on meaning variance theory. However he points out that meaning variance theory does not yield desirable consequences. He sketched various undesirable consequences to establish his position of meaning invariance and suggested an alternative account for the comparison of theories through appeal to first level and second level invariance. Thus he holds his position in opposition to widely influential views of Kuhn which maintains that transition from one scientific paradigm to another force a change in the meaning of the terms employed which is radical enough to preclude the possibility of comparison of scientific theories from different traditions. For example according to Kuhn and also by Feyerabend, the meaning of "mass" (among other terms) has radically and incommensurably changed meaning in the transition from classic to relativistic mechanics.[1]

The dependence on velocity and convertibility with energy are built into the relativistic concept of "mass."

Kordig finds that the philosophical interpretation of such example is implausible so a deeper objection against meaning variance thesis was raised by him which are being explained as under.

The first methodologically implausible consequences of the doctrine of radical meaning variance is that if it were true then no theory could be consistent or contradict with another. (When a new theory T1 emerges to replace an old one T, the terms involved both theoretical and observational will change in such a way that there will be an elimination of old meaning - this is the view of the proponents of radical meaning variance theory.[2] Here two different incommensurable concepts will emerge out by the same term employed in both cases. Hence T1 and T could not contradict each other or mutually be inconsistent.

For further elaboration Kordig quoted the Bohr theory of atoms which assumes that electron revolves about the nucleus of an atom in such a way that their orbital angular momentum is quantized (it is a whole multiple of $h/2\pi$, where h is Plank's constant); it also assumes that energy is radiated or absorbed by the atom only when an electron jumps from one stable orbit to another and that this energy is also quantized. When the Bohr theory claims that angular momentum and radiant energy of electrons cannot have continuous values but must be quantized, it denies the assumption of classical electrodynamics that angular momentum and radiant energy of electrons can have continuous values. I will further explain the classical electrodynamics in comparison to

Bohr's theory of atom to present the ground reality towards the claim of Kordig. According to classical electrodynamics the angular momentum of moving electron around the nucleus is not quantized (i.e. it is not integral multiple of $h/2\pi$). When electron jumps from lower energy level to higher energy level there is loss of energy in the form of radiation which is termed as radiant energy but in this case it will be quantized. It can be presented mathematically, $\Delta E = h\nu = E_2 - E_1$ where E_1 is the first energy level and E_2 is the second energy level. Here energy is proportional to frequency 'ν'. So it is continuous. But Bohr modified classical electrodynamics that angular momentum of a moving electron around the nucleus of an atom is not continuous but discontinuous (quantized) when electron jumps from lower energy state to higher energy state there is a loss of energy in the form of radiation and this radiant energy will be quantized because $E=nh\nu$ where n=1,2,3,---. This energy will be only in the whole number not in fraction. So we see that the terms such as angular momentum, radiant energy used in Bohr's theory would be held to have different meanings from those in classical electrodynamics. Indeed they are to express incommensurable concepts, thus they could not contradict one another if the radical meaning variance thesis is true.

The second methodologically unacceptable consequence of the doctrine of radical meaning variance is an extension of the first, which can be stated in the following way. If the doctrine were true then each scientist would be effectively isolated within his own system of meanings. Each of these meaning would be radically different from those of scientists within other traditions or from those of scientists holding other theories. True communication from one such system to another either in agreement or disagreement would be impossible. Each as Scheffler points out would be "trapped in the web of his meaning".[3]

Kuhn maintains that competing paradigms are addressed to radically different problems.[4] They incorporate radically different standard and even radically different definition of science. They are based on radically different meaning.

But if this is so in what sense could such paradigms be said to be in competition? How could they be either rivals or alternatives? To maintain that they are in competition is to place them within some common framework, which has comparative and evaluative standard applicable for both. It is to consider them as oriented in somewhat different ways towards the same purpose and scientific goals. And it is the invention of alternatives for the purpose of mutual criticism which is central to Feyerabend's own positive methodology:

> You can be a good empiricist only if you are prepared to work with many alternatives theories rather than a single point of view and experience.[5]

Given the radical meaning variance position, two different theories are radically different in meaning. It is thus hard to see how they could function as alternatives to each other or serve to criticize other, just as sociological theory and quantum theory which are radically different in meaning are neither alternative to each other nor serve to criticize one another. Feyrabend thinks that adopting the radical meaning variance position enables us to come closer to and more fully reach his goal. On the radical meaning variance view, theory displacement in science is held to affect observational as well as theoretical categories and notions. It follows that apparent sharing of observational terms by theoretical opponents is really a delusion. If we are to understand another's observational or experimental claim, it is held that we must first accept his theory. Further it is held that if we are to understand another scientist's language we must share his theory; this means we must share certain deep features of his thought-world, his outlook, expectations and beliefs for it is only in terms of these. It is claimed that his language can be rendered intelligible. True communication between holders of different scientific theories thus becomes impossible. For a scientist of one tradition to significantly converse with an opposing theorist from another tradition on neutral ground becomes impossible. Kordig thus holds the view that radical meaning variance, as Feyrabend thinks is not methodologically desirable.

There is a third methodologically undesirable consequence of the doctrine of radical meaning variance. If the doctrine were true it would be difficult to see how one could learn a new theory. He could not learn it by having it explained to him using any scientific terms whose meanings he understood before he learned the new theory. Consider the term mass, velocity, and energy, which are used, in relativistic mechanics. The meaning of each of these terms, given the doctrine of radical meaning variance, is theory Laden. As Hanson would say, "The entire conceptual pattern of the game is implicit in each term"[6]. If so, then, in order to know what the terms of relativistic mechanics mean, I must know relativistic mechanics or at least its central principles. One of these central principles can be roughly expressed like this. 'Mass' is a function of velocity and is convertible with energy.

But it is hard to know how I could understand what the above expression asserts unless I already to some degree know what the term's man, velocity, energy mean. While radical variance theorists would hold that to learn what the terms mean I must learn the theory. To learn the theory, however, I must learn its central principles. But to learn the latter, it would seem that I must know what the terms involved mean. Given radical meaning variance, this circularity is vicious. In trying to circumvent, it is useless to appeal to what the terms mean in different theories. What they mean in any two theories is held to be radically different and incommensurable. Therefore Kordig says that one could not use any term whose meanings have been understood in order to learn the meaning of the terms in relativistic mechanics. Thus if the radical meaning variance accounts were correct, most scientific looks would therefore end up useless in principle.

So the finding of Kordig is that it is difficult to see how one could in any sense learn a new theory by adopting the general radical meaning variance position.

There is a fourth methodologically undesirable consequence of the doctrine of radical meaning variance position. If the doctrine were true, no scientific theory could be tested or falsified by any observation reports. Feyrabend and Kuhn would agree that without the help of other theories, no such theory could be falsified.[7]

As Feyrabend puts it:

> One most important point of agreement is the emphasis which both of us (Kuhn and Feyrabend) puts upon the need, in the process of the refutation of a theory for at least another theory.[8]

There exists facts that can not be unearthed except with the help of alternatives to the theory to be tested and that become unavailable as soon as such alternatives are excluded.[9]

Both the relevance and the refuting character of many decisive facts can be established only with the help of other theories that, although factually adequate, are not in agreement with the view to be tested. Empiricism demands that the empirical contents of whatever knowledge we posses be increased as much as possible.[10]

This implausible if the doctrine of radical meaning variance is correct, indeed it is inconsistent with the doctrine.

A different theory T1 could not be used to show that observations exist which do not satisfy T. The concepts of T1 would be held to be incommensurable with those of T. Thus, satisfied prediction statements expressible in T1 could not be used as Feyrabend wishes them to be used. If they could then T and T1 would not be incommensurable which is contrary to the radical meaning variance position.

There is a fifth methodologically undesirable consequence of the doctrine of radical meaning variance. If it were true then there would be no sense left to the notion of a rational progression of scientific viewpoints from age to age.

We had seen in the previous undesirable consequences related to the doctrine of radical meaning variance theorists that:

1). Two different theories could neither agree nor disagree (contradictory).

2). That each scientist is effectively isolated with his own and unique system of meaning.

3). That no theory can be falsified or tested.

4). That scientists could not learn new and different scientific theories.

Thus, Kordig finds that there is no sense to the customary notion of the rational progression of scientific viewpoints from age to age. He holds that when scientists choose one theory over another we could not claim that this choice constituted progress. For example we could not claim that the scientific community's choice of Einstein's theory over Newton's theory and of Kepler's theory over Brahe's theory constituted progress. Kordig examined this consequence and concluded that the problem arises because Kuhn gives no reason consistent with the rest of his positive which could serve as grounds for accepting one paradigm as better or more acceptable than another. Given his general position one could not say, in any ordinary sense, that progress is made when another replaces one paradigm through scientific revolution. Why? Different paradigms radically disagree as to what are the facts, the problems faced, and the standards which the successfully theory must meet. A paradigm change brings about changes in the standards governing permissible problems, concepts and explanations.[11] Kuhn's position indeed tends towards the conclusion that the replacement of one paradigm by another is not commutative but is more replacement, mere change. If this is so two different paradigms could not be judged according to their ability to solve the same problems, deal with the same facts or concepts, or meet the same standards, for all of these are radically different for different paradigms. Such a conclusion is, however, inconsistent with the positive part of Kuhn's methodology. In discussing how paradigm disputes are finally resolved,[12] Kuhn draws attention to "two all important conditions" which a new and successful paradigm will satisfy. First it will, resolve some outstanding and generally recognized problem that can be met in no other way. And second it will, "preserve a relatively large part of the concrete problem solving ability that has occurred to science through its predecessors". It will preserve a great deal of the most concrete parts of past achievement. However, neither of these, "all these important conditions" could be met if the rest of the Kuhn's interpretation were correct. As scheffler has correctly noted:

> Such conditions of evaluation contradict the main thesis appealing to the history of science, namely, that paradigm change in science is not generally subject to deliberation and critical assesment.[13]

So the consequence according to Kordig from methodological point of view for the radical meaning variance position held by Kuhn is undesirable. He has a second sort of objection to the claim that if the radical meaning variance thesis were true then scientific change could not constitute progress.

Toulmin claims, [14] in effect, that there is a special sense of progress and cumulative in which the radical meaning variance position does not entails that scientific change is non-cumulative or non-progressive. After accepting the radical meaning variance thesis as to both observational and theoretical term, [15] he exhibits some sensitivity to the problem ---how do we know which presupposition to adopt? Certainly, explanatory paradigms and ideals of natural order are not true or false in any naïve sense. Rather they take us further (or less far) and are theoretically more or less fruitful.[16] Given Toulmin's radical meaning variance position, it would be doubtful as Shapere has pointed out,[17] whether there are shared ideals or standards which are invariant with respect to scientific revolutions. Therefore it would be doubtful whether different paradigms could be judged to be "more or less fruitful" in accomplishing common tasks. For the same reason it would be doubtful whether there were common jobs that one paradigm could take us further towards than another. So the question that how theories can be judged against one another and how the replacement of one theory by another can be said to constitute progress or an advance remains unanswered.

The sixth methodologically undesirable consequence of the doctrine of radical meaning variance position is perhaps the worst. The doctrine is demonstrably untenable because of a self-referential problem.

For this, Kordig examines the position that there is no objectivity in science. Consider the claim that the choice between only scientific theories is a matter of taste (as hold by Feyrabend) where, "neither proof nor error is at issue (Kuhn). Such a restricted claim is nevertheless problematic. It leads to an unjustified dualism. On the one hand we are supposed to hold that, "science is a subjective enterprise whose concepts and domain are theory laden. On the other hand we are supposed to also hold that the philosophy of science is an objective enterprise whose concepts and domains are not theory laden. People who adhere to the earlier claim would to avoid the self referential problem and have to maintain that their own views of scientific change are uninfluenced by the fact that they are Kuhnians that is they would have to hold the latter claim. But what is the difference between these two domains? The answer is that non-theory laden facts are relevant to the philosophy of science but not to science. Kuhn's and Toulmin's advocation of a purely descriptive methodology for evaluating rival scientific theories are the last example here.[18]

Therefore the doctrine of radical meaning variance is either demonstrably untenable or leads to neopositivistic aspects.

So Kordig has suggested an alternative account which has distinct virtues and advantages. He has suggested that comparisons of different theories are possible since it is possible for them to be some shared meaning of the terms involve.

But he urged that comparisons of different theories are in fact made through appeal to shared principles and meaning at both a "first" and "second" level. By shared meaning at a first level Kordig means shared extension of terms employed by rival theories and by shared principles and meaning at a second level he means shared regulative principles which scientists require of successful theories and which guide their choice among alternative theories. Kordig argued that the "first level" in variance usually occurs in scientific transition.

Let us consider the example of observational or experimental invariance, namely the transition from Galilean physics to Newtonian physics. Nagel thinks that the former science is reduced to the latter, [19] which Feyrabend denies it.[20] . As Feyrabend, [21] correctly notes, Galilean physics dealt with the motion of material object (falling stones, penduli, balls on an inclined plane) near the surface of the earth. Its subject matter was a terrestrial object. It is possible to distinguish the subject matter of the Newtonian and Galilean science. The latter was concerned only terrestrial phenomenon and the former dealt also with the celestial phenomenon. Thus the subject matter of two sciences says are not coextensive. But since Newtonian physics also referred to material objects near the surface of the earth, the subject matter of Galilean physics is a subset of the subject matter of Newtonian physics. The objects referred to by name of the terms employed by T_1 (Galilean physics) are also referred to by some of the terms employed by T_2 (Newtonian physics). In the transition from T_1 to T_2 there is observational invariance and thus refers to extensional meaning invariance. As Nagel correctly notes in his discussion of the reduction of T_1 to T_2 , their subject matter are in an obvious sense homogeneous and continuous; for it is the motion of the bodies and determination of such motions that are under investigation in each case.[22] Using Feyrabend own words, we can describe the neutral observational objects in terms which are neutral to T_1 and T_2 . Both T_1 and T_2 refer to material objects such as falling stones, penduli, balls on inclined planes etc, each of which are near the surface of the earth. None of the terms used in this description are peculiar to only T_1

and T_2, nor does the description presupposes meaning invariance between T_1 and T_2 . We have assumed only the ordinary English language meaning of these terms - whether or not they have the same meaning as their typographical counterparts, if any, either T_1 and T_2 . Thus in order to establish whether our description is correct and whether there exists observational invariance between T_1 and T_2 , one need not presuppose meaning invariance between T_1 and T_2 and our description, indeed, is correct as every historian of science would recognize. Therefore, there is observational invariance between T_1 and T_2. And hence there is also extensional meaning invariance between Galilean and Newtonian physics. Galilean idea of mass, acceleration, force were applicable to all bodies located near the surface of the earth, the Newtonian idea of gravitational force included these bodies within its range and of course added celestial bodies as well. Margenau notes this point well:

> The Galilean ideas of mass and acceleration could be applied to a great variety of bodies, namely all those located near the surface of the earth. However, Newton's discovery of the law of universal gravitation was more extensible; it included within its range of celestial bodies. Bay seizing upon the idea of a gravitational force, Newton provided a concept of impressive width, thereby significantly advanced the science of mechanics.[23]

This overlap between the ontologies of Galilean and Newtonian physics is non-trivial. The class of all terrestrial objects is not a trivial or an insignificant class. And this class compromises the overlap of the common objects dealt with by both of these physical theories. Terrestrial object is in each theory a correct answer to the question what moves? A term that illustrates non-trivial is meaning invariance in both theories, therefore, inertia. There are other terms also which illustrates non-trivial meaning invariance. In both theories terrestrial objects undergo displacement. Similarly, in each theory terrestrial objects have mass, under go acceleration and undergo velocity. There is, therefore, also some non-trivial meaning invariance with respect to these phrases. There are our two examples of observational invariance for first level.

Now we have to see the findings of Kordig for the second level discourse. He finds Kuhn's position regarding this as fallacious because Kuhn suggests that sharing of second order standard is impossible.[24] Kuhn feels that acceptance of a paradigm entails acceptance of governing standards or criteria. One can then use these to justify his acceptance of the paradigm against its rivals. Because of this Kuhn maintains that different paradigm employ different standard at the second level of scientific discourse. He concludes that each paradigm is, in effect, fails justifying and provides science not only with a map but also with some of the directions essential for map making.[25] However the standards used to evaluate paradigms themselves are different in nature and function from the standards, internal to a paradigm. To say there are internal standards involved in a paradigm is just to say that the paradigm may be understood as defining, within some scientific domain, a range of legitimate problems along with approaches to and forms of solution. However, no set of mapping directions employs how it is itself to be evaluated in comparison with alternative sets rather as Scheffler notes, [26] no such set implies that it is itself superior to its alternatives. Thus it is in this sense, each paradigm is not self-justifying.

So kordig suggested the second order standard for such task. These standards might then be used by philosopher of science as a philosophical rationale for the evaluation of a paradigm against one another. In the second order sense each paradigm is not self-justifying contrary to Kuhn, because his argument has not precluded neutrality or objectivity from playing a role in paradigm evaluation. He has not demonstrated that scientific transitions consist only in non-cumulative persuasions and conversions. Kordig holds the view that at a second level standard, there should be some invariance with respect to scientific change. Shared second level standards are needed, and used, in the business of accepting, rejecting, and evaluating rival or competing theories. They serve to regulate the choices among rival theories. He has briefly described several guiding principles. None is absolutely invariant each may change in time. But some accounts along with some illustrious from the history of science should show that they need not change with change in scientific theories, that changes in them proceed very slowly, and they are in fact usually invariant with respect to scientific transitions. It is through appeal to such shared standards that the shift of allegiance in the scientific community usually occurs. By drawing the work of Margenau,[27] Kordig has briefly discussed the following.

a. Empirical confirmation: A theory is confirmed of its consequences via the rules of correspondence. Further the theory that has sustained many circuits of empirical confirmation is usually promising for scientific acceptance. The range of application of confirmation theory is designed to include any scientific theory.

b. Logical fertility: Margenau expresses it by saying that hypothesis of scientific theory should obey logical laws.[28] If, for example, a scientific theory is logically inconsistent then this is ground for its rejection. Scientific theory should be coherent. Logical coherence is one of the regulative aims of science. This requirement can be shared by and used to evaluate different scientific theories. It is a demand, which is usually invariant with respect to changes of scientific theory. Even with respect to quantum mechanics where much valued logic has been considered, logical consistency has not been given up.

c. Extensibility: Scientific theories should be extensible to as large as possible. Other regulative principles being equal, the more extensible is the better theory. On this score, Newtonian mechanics, which dealt with both terrestrial and celestial objects, is a better scientific theory than Galilean mechanics which dealt only with the former objects.

d. Multiple connections: Scientific theory should be organized and systematic. That is the constructs used in scientific theories should be multiple connected. Scientific hypothesis should be adhoc. Hempel[29] and Grubaum[30] stresses this point well.

e. Simplicity: It is often used as a regulative principle in evaluating rival scientific theories, is beyond doubt. This notion has often been appealed to as a basis for choosing among rival theories. The Copernicus revolution is a case in point. Copernicus, by placing the sun at the center of the planetary universe, was able to reduce the number of epicycle from 84 to 30. This eliminated a large number of unrelated epicycles, which has previously been needed to explain the same observation. Perhaps because of this simplicity Copernican astronomy was more systematic and less adhoc than Ptolemic astronomy. And this too is a factor in comparing these theories, as Runder aptly puts it, "system is no more adornament of science, it is very least."[31] Simplicity is being deliberately conducted in a way, which would permit the application to more than one theory.

f. Causality: Kordig argues with Margenau that causality is a metaphysical requirement. It demands that constructs should be so chosen as to generate causal laws. Holding the view of Margenau, Kordig regarded causality as a property of physical law and not as a relation between single observations. This regulative principle is met by both Galilean and Newtonian mechanics and by the scientific theories of both Brahe and Kepler. The principle was not abandoned in this transition, it continued to be employed and was in fact fulfill by the laws of these theories. It was, therefore, invariant with respect to these scientific transitions.

Thus Kordig holds that objectivity is an ideal, which is in fact employed in the scientific practice. Science is a systematic public enterprise that can be justified by reference to second level standards, logic, and empirical facts. Part of the purpose of science is to formulate truths about the natural world in a simple, comprehensive systematic and intelligible ways in which nature becomes explainable, predictable and controllable. The success of any particular scientific theory is a measure of how far, and how successfully, it contributes to the realization of the general second level aims of science. If significant first level sharing occurs between T and a competitor T^1 and if T takes us further in the direction of the above second level aims than T^1, then T should be accepted and T^1 rejected.[32]

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Mark. A. Stone

So far I have discussed the views of Lakatos, Laudan and Kordig. In this section, I shall examine the views of Mark. A. Stone. He holds that Kuhn and Lakatos are against the traditional views about science. Their criticism share two themes; first, that a scientific theory is not tested alone against empirical result and second, that one does not reject a theory without a successor at hand. In Kuhnian notion it is the paradigm of the theory that is either accepted or rejected while in Lakatos' terminology it is the research program of the theory that is either accepted or rejected with the condition that a successor must be available before the rejection of a paradigm or research program.

He agrees with the view of F. M. Akeroyed who criticizes and holds opinion that there are occasion in the history of science when a research program is simply rejected and indeed rationally rejected despite the absence of a successor.[1] Mark Stone says that Kuhn is subject to the same criticism with respect to his favorite historical example of Copernican revolution. In his subsequent writings, Kuhn has identified two main sources of paradigm; the disciplinary matrix and the exemplars of a scientific community.[2] This disciplinary matrix consists of those rules, some explicitly stated and some only implicitly understood that provide methodological guidelines to how research and experiments will be conducted and that will determine what will count, as legitimate problems for investigation by the community as well as criteria for legitimate problem solutions. In addition, the disciplinary matrix includes the metaphysical beliefs shared by the community about what sorts of entities are present in and what sorts of processes are at work in the world. Exemplars constitute a body of concrete examples of successful problem solutions that provide the community with an understanding, largely implicit of its disciplinary matrix and the theories that are formulated within that disciplinary matrix. So the use of the term paradigm refers to the conjunction of disciplinary matrix-exemplars theory that constitute the discipline of a particular scientific community. Kuhn's evaluation is based in part on a linguistic thesis and in part on an epistemic thesis. The former asserts that meaning of a paradigm is holistic, one can not understand the belief or a assertion of a scientific community in isolation rather within a whole interrelated network of a paradigm. While the latter asserts that there is no paradigm independent foundation for theory, grounds for believing a theory and grounds for believing that certain empirical results require explanation and are based entirely on criteria internal to the paradigm of that theory. This is the basis on which Kuhn rejection of prior verificationist / falsificationists approach lie. He summarizes his criticism of verificationism as follows:

> When paradigm enters, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense.[3]

Kuhn's rejection of the principle of falsifiability is more complex. Kuhn recognizes that no single observation that apparently contradicts the prediction made by the theory suffices as grounds for rejecting a theory. One can not neatly separate those observations that constitute problems yet to be solved from those that come to be recognized as genuine anomalies. As Kuhn says:

> There are, I think only two alternatives; either no scientific

> theory ever confronts a counter instance, or all such theories confront counter instances at all times.[4]

Kuhn's argument here provides the basis for his claim that it is paradigm as a whole and not theories in isolation that are accepted or rejected. He further argues that a scientist does not reject one paradigm without simultaneously accepting another. Since Kuhn regards the possession of a paradigm as a necessary condition for practicing science at all, he concludes that,

> "To reject one paradigm without simultaneously substituting another is to reject the science itself. That act reflects not on the paradigm but on the man. Inevitably he will be seen by his colleague as the carpenter who blames his tool."[5]

He further claims that,

> Once it has achieved the status of a paradigm, a scientific theory is declared invalid only if alternate candidate is available to takes its place. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigm with nature and with each other.[6]

But there is an important question here, if a scientist never abandons one theory until a successor is at hand, then where do successor theories come from? If scientists always accept the current paradigm until presented with an alternative, then it is unclear who will do the work to formulate an alternative. To answer this puzzle we must look in more detail at the process of scientific discovery. The answer will show that at least in some cases, scientists can and must reject one paradigm without ready successor.

At this juncture Mark A. Stone has established his sharp opinion and argued in the following way; He divided scientific discoveries into three types. First, at times revolutionary discoveries occur by accident. It has happened that a scientist will by chance stumble across a phenomenon so striking that he is compelled to adopt a new paradigm in-order to assimilate the new phenomenon. Mark Stone calls this spontaneous discovery. Second, at times revolutionary discoveries occur unnoticed. A scientist makes what he thinks is only a small addendum to some well established theory, and only later comes to realize that the implications of his modification require abandoning his previous paradigm. Mark Stone calls this implicit discovery. Finally, a scientist may perceive a felt need for a new discovery, set about to find it, and succeed. Mark Stone calls this directed discovery.

The discovery of x-rays is a typical example of spontaneous discovery. While doing routine experiments with cathode rays the physicist Roentgen observed that a barium platino cyanide screen in the laboratory began to glow whenever cathode rays were discharged. Weeks of investigation by Roentgen produced a rudimentary theory of x-rays. At first he refused to believe that cathode ray emissions were in fact responsible. However: further investigation, they required seven hectic weeks during which Roentgen rarely left the laboratory- indicated that the cause of glow came in straight lines from the cathode ray tube, that the radiation cast shadows, could not be deflected by a magnet, and much else beside.[7]

Thus Roentgen found himself in a situation in which the new phenomenon could only be understood with the aid of the new paradigm. To accept a new phenomenon was in fact to accept a new paradigm.

The process of implicit discovery accords with Kuhn's view. Mark Stone finds the example of this process in the origin of quantum theory. In 1900 Plank was studying the phenomenon of black body radiation. A body such as an iron bar, when sufficiently heated goes through a spectrum of changes in color for dull red to bright white. Plank wanted to understand the relationship between the energy absorbed by the body and the radiation emitted by the body. Plank saw a connection between his problems and a problem to which Boltzamann had offered a new solution. Boltzamann was concerned with gases, not solid bodies. His question was: given the known average velocity of the molecules in a gas, what proportions of the molecules are moving at some multiple of that velocity? Boltzmann solution was original because of its application of probability theory. He imagined a line segment scaled to the total kinetic energy of the molecules in the gas, so that one end point of the segment is zero and the other end point is E, where E stands for the total kinetic energy of the molecules. He then considered this segment be divided into finitely many smaller segments of equal size. What are the possible distributions of molecules among these cells, given that the sum of the kinetic energy for all molecules cannot exceed E? Boltzmannn was able to show that only certain distributions satisfied these restrictions and using probability theory he was able to show what the likelihood was that any given distribution would be represented in a gas.

Plank made some theoretical assumptions that allowed him to treat the problem of black body radiation like Boltzmann's problem. Plank theorized that a body was filled with what he called resonators, which resonate radiation only at a particular frequency, and that the body as a whole would be filled with resonators covering the full operation of frequencies. Dividing the energy line segment into smaller segments as Boltzmann did, Plank then could ask what proportion of resonators fill along each smaller segment and apply probability theory to obtain a solution again just as Boltzmann had.

This much of the story fits exactly the pattern of what Kuhn calls normal science. Certainly Plank saw nothing revolutionary in his theory. Indeed Plank did not immediately see any thing significant in the one additional restriction that he had to place on his theory that was not part of Boltzmann's solution: the unit sizes into which the energy line could be divided were not arbitrary, but were in fact dependent on the number we now know as a Plank's constant.

In order to understand Plank's solution one had to accept that there were discrete jumps in the energy level of an atom. This result was simply unintelligible by the standards of the existing paradigm. Assimilation of Plank's solution therefore required a new paradigm. The effect of implicit discovery is thus similar to the effect of spontaneous discovery. One either accepted the new problem solution, or thus accepted a new paradigm. There is no rejection of the older paradigm without simultaneous acceptance of the new paradigm.

Mark. A. Stone holds that the significance of spontaneous and implicit discoveries are accidental and when he turned his attention towards directed discovery, he sees that Kuhn has overstated his statement. The historical example that Kuhn discusses most frequently, the Copernican revolution is an instance of this discovery. Copernicus the very famous astronomer judged that the Ptolemaic theory of celestial mechanics is unacceptable and he was firm in his conviction that

Ptolemy's theory had to be rejected. In his own words:

> The mathematicians are unsure of the movements of sun and moon that they cannot even explains or observes the constant length of the seasonal year. Secondly, in determining the motion of these and other five planets, they use neither the same principles and hypothesis nor the same demonstrations of the apparent motions and revolutions.[8]

So Kuhn says:

> "For the first time a technically competent astronomer had rejected the time honored scientific tradition for reason internal to his science, and this professional awareness of technical fallacy

inaugurated the Copernican revolution."[9]

So Mark Stone sees a clear refutation of Kuhn's own thesis that scientist never rejects one paradigm until a successor is at hand and yet Copernicus even by Kuhn's own admission has done precisely. Thus Mark Stone has established that Kuhn has made two mistakes in overlooking the significance of directed discoveries. First he has conflated two different sense of reject. Second he has failed to notice that within his own framework there is a room for an account of falsifiability that makes sense of directed discoveries. On the one hand "reject" can mean to find unacceptable. On the other hand "reject" can mean no longer make use of it. First is called by Mark Stone as epistemic rejection where rejection entails a change in belief. The second is called as pragmatic rejection, where rejection entails a change in action. Thus Mark Stone feels that Kuhn requires an alternative account of falsifiability and rejection for cases of direct discoveries. So according to him Kuhn's views are threatening to the rationality of science[10] as his claim between the case of Copernicus and Ptolemy has failed because of methodological stricture of Ptolemic paradigm was that planetary motion be accounted for, in terms of circular motion while Copernicus argued that Ptolemic theory had in effect failed to adhere the stricture.

So the view being established here entails that Kuhn's old paradigm if altered requires new one has not a valid ground according to Mark Stone and he further says that if the revolutionary discovery is either spontaneous or implicit then the results are happened by chance and if the discovery is directed then it must be preceded by a rational procedure.[11]

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John Watkin

John Watkin confronted with Kuhn's account of Normal science and ultimately disproved that Normal science constitutes the essence of science.

He argued that Normal science could not have the character as Kuhn ascribes to it that it is capable of giving rise to extraordinary or revolutionary science.

Later by taking into account the Kuhn's comparative evaluations of Normal and extraordinary science on the supposition that history of science does in fact display a Normal science-Extraordinary science cycle, Watkin challenged this supposition in different form. His objection was concerned with the possibility of the emergence of a new paradigm at the end of a period of Normal science and concluded that the new paradigm never could emerge from Normal science as characterized by Kuhn.

For this Watkin recapitulated Kuhnian thesis concerning paradigm change which ultimately leads to meaning change in scientific theories.

1. It is the nature of a paradigm to enjoy a monopoly in its hold on a scientist's thinking. A scientist cannot entertain a rival paradigm while under the sway of one paradigm. If he has started toying with a rival paradigm, then the old paradigm is already defunct for him. This he called paradigm-monopoly thesis.
2. There is little or no interregnum between the end of the old paradigm's reign over a scientist's mind, and the beginning of the new paradigm's reign. A scientist does not flounder around for any substantial length of time with no paradigm to guide him. He abandons one paradigm only to embrace a new one. Watkin calls this non-interregnum thesis.
3. A new paradigm will be incompatible with the paradigm it supercedes and further Kuhn claims

that it will be incommensurable with the old one.[1] Watkin calls this thesis as a clash between the old and new paradigm.

4. From the conjunction of the above three theses, it follows that a scientist's change over from an old paradigm to a new; one must be swift and decisive. Kuhn emphatically endorses this application. He says that paradigm switch is a relatively sudden and unstructured event like the gestalt switch.[2] So Watkin calls this as Gestalt switch thesis.

5. Kuhn's view allows that it may take quite a time for a paradigm, once invented to gain general acceptance. But, how long may it take the original inventor to put together the rudiments of the new paradigm. He says that we must remember that the new paradigm is immediately powerful enough to induce our scientist to turn against the well articulated and unrefuted paradigm that has dominated his scientific thinking. This means

that new paradigm at the outset must be large and definite enough for its striking potentials to be fairly apparent to its inventor. If that is so, the instant-paradigm thesis according to Watkin seems to be barely credible on psychological grounds. He also says that there seems to be a certain internal incoherence in Kuhn's version of his thesis and here Watkin[3] calls that the paradigm monopoly thesis must go.

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Chapter-4

Chapter 4

Conclusion

Science is not united by its subject matter but rather by its methodology. What sets the scientific approach apart from other modes of acquiring knowledge are the assumptions, upon which it is based and its methodology. Thus the scientific methodology is a system of explicit rules and procedures upon which research is based and against which claims for knowledge are evaluated. This system is neither closed nor infalliable. Rather the rules and procedures are constantly improved; scientists look for new methods and techniques of observations, inference, generalization and analysis. Thus it follows that method of science is more stable and more important to the philosopher of science. The method makes possible the noting and correction of errors by continued application of itself and enhances the internal consistency of claims for empirical knowledge.

Since, the central problem in 20^{th} century philosophy of science has been the nature of meaning in scientific theories. For this purpose, I have organized my work in such a way so as to examine the adherents of this view, particularly Kuhn and its aftermath effect.

The chapter two entails in detail the Kuhn's notion of meaning variance, which emerged as a major alternative to logical positivism. Within the framework erected by logical positivism, change, comparison of meaning, presented no problem rather the terms used in a theory whose meaning can be given independently of the theory, remains constant across the theory. In contrast to positivist's position Kuhn maintains that meaning of a term that occurs in a scientific theory changes when theory is modified or replaced by another theory. Thus by realizing the ultimate inadequacy or irrelevance of logical positivism in understanding the growth of science, Kuhn sets about the task of giving an explanation of the way in which it comes about. It is clear in his program that historical evidences lie in the background for the articulation of the framework, which he employed.

Another significant feature is that in contrast to normal science, Kuhn views revolutionary science as the abrupt development of a rival paradigm that can be accepted only gradually by a scientific community. It is then, established that, degree of meaning variance rests upon paradigm shift. Further the work of Kuhn has led to extensive discussion on the problems of theory change and it is important to be clear that there are two distinct problems involved; the analysis of the grounds for deciding to abandon one theory and replace it with another and the clarification after the fact of the relation between the two successive theories.

In the next chapter (chapter 3) the views of the few eminent post-Kuhnian (Imre Lakatos, Larry Laudan, C.R.Kordig, Mark.A. Stone, John Watkin) thinkers have been discussed. How far have they established their position in contrast to Kuhnian notion?

Imre Lakatos does not fully agree with Kuhn's notion of scientific progress through paradigm shift. According to him the rationale of scientific revolution lies in the progressive research program. He says that science is not simply the achievements of hypothesis, a series of conjectures and refutations but rather a research program. He has discussed the problems of objective appraisal of scientific growth in terms of progressive and degenerating problem shift in series of scientific theories. For example Debroglie's paper came at the time when Bohr's program was degenerating.

Lakatos was of the view that one must stay with a research program until it has exhausted all its heuristic power, that one must not introduce a rival program before everybody agrees that the point of degeneration has probably been reached. According to him the history of science has been and should be a history of competing research program. When two research programmes compete, their first ideal models usually deal with different aspects of the domain. (For example, the first model of Newton's semi corpuscular optics described light refraction and the first model of Huyghen's wave optics described light interference). As the rival research program expands they gradually encroach on each other's territory and the version of the first (research program) will be inconsistent with the version of the second.

After Lakatos, the next eminent post-Kuhnian thinker to whom I have incorporated in chapter three is Larry Laudan. We can now observe his implication of model of scientific change in contrast to Kuhn and Lakatos. Laudan says that Lakatos' research program like Kuhn's paradigm is rigid in their hard core structure. So he turned to explore an alternative model of scientific progress that is "research tradition" which provides a guideline for the development of scientific theory. Laudan finds that both Kuhn and Lakatos are committed to the views that there is two radically different types of science. For Lakatos it is immature and mature science; for Kuhn, it is "pre" and post-paradigm science. For Kuhn transition occurs when one paradigm establishes monopoly over the field and when normal science ensues. For Lakatos, a science reaches maturity when a scientist focuses entirely on the mathematical articulation of research programs. Thus for both Kuhn and Lakatos, it is emergence of paradigm or research program.

Laudan had established that he hardly finds any characterization of mature science in both (Kuhn and Lakatos) which will do justice both to history and rationality. So rather he is suggesting that a scientific revolution occur when a research tradition reaches a point of development where scientists in the field feel obliged to consider it seriously as a contender for the allegiance of themselves or their colleagues.

The next eminent thinker who has been taken into account in this chapter is C.R.Kordig. He holds his position in opposition to widely influential views of Kuhn, which maintains that shifts of one paradigm to another force a change in the meaning of terms employed, which precludes the possibility of comparison of different scientific theories. Kordig holds that during a revolution when a scientist embraces such a new paradigm, he does not interpret. Rather, according to Kuhn, he experiences a Gestalt shift, which is a sudden and unstructured interpretation. Kordig further argues in favor of justification of scientific change, which rests upon different sorts of invariance. He also sketches an account of comparison for different scientific theories to avoid the shortcomings of meaning variance theorists like Kuhn.

The another post Kuhnian philosopher included here is Mark A. Stone. He argues that Kuhn is mistaken in his claim that scientist never rejects a paradigm without simultaneously accepting a new paradigm. Mark stone on the other hand says that science rejects a paradigm despite the absence of a successor. Thus Kuhn was subjected to criticism with respect to his historical example of Copernican revolution. Second thing, which was raised by Mark Stone against Kuhn is about the types of scientific discoveries. He differentiated scientific discovery into three types: spontaneous; implicit & directed and finally says that Kuhn thesis holds for spontaneous and implicit discovery but not directed discoveries. Thus he established that in the former two sorts of discoveries, results are happened by chance but in accordance with the last one that is directed discovery result is preceded by rational procedure.

John Watkin also discussed the problem of meaning variance in the form of paradigm-shift (Kuhn). He objected the possibility of the emergence of a new paradigm as characterized by Kuhn and says that his paradigm monopoly thesis should go.

Thus there is no lack of suggestion for the criteria used by scientist in making their choice which further establishes that the history of science is a tale of multifarious shifting of allegiance from theory to theory.

Among the philosophers who have been discussed, *I find Kuhn's approach of "paradigm shift", a dependent criteria for meaning variance is more successful, more appealing, undefeatable and highly conclusive.*

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